

LETTERS TO THE EDITOR.

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Biometry and Biology: A Reply to Prof. Pearson.

IN reply to Prof. Pearson's letter in NATURE of September 6 (p. 465), I desire, in the first place, to express my extreme regret if the criticism which I ventured to offer on biometrical work in my address at York has caused pain in a quarter where I should least desire to give offence. Had I foreseen that this was likely to happen I certainly should have refrained from making any criticism on that occasion.

Prof. Pearson wishes me to explain how Dr. Pearl's paper, "A Biometrical Study of Conjugation in *Paramæcium*," an abstract of which appeared in the Proceedings of the Royal Society (B, 518, p. 377), lays him open to the advice that he should make sure that the problem he seeks to elucidate is sound from the standpoint of biology. I think that there is no course open to me but to comply.

Dr. Pearl states that his work on *Paramæcium caudatum* was undertaken for the purpose of obtaining answers to the questions:—

"(a) Is the portion of the *Paramæcium* population which is in a state of conjugation at a given time differentiated in respect of type or variability, or both, from the non-conjugating portion of the population living in the same culture at the same time?

"(b) Is there any tendency for like to pair with like ("homogamy") in the conjugation of *Paramæcium*, and if so, how strong is this tendency?"

In making the first inquiry, and in dealing with it, Dr. Pearl appears to ignore the fact that the differentiation of the conjugants of this species is already well established. Maupas (*Arch. de Zool. exp.*, ser. 2, T. vii., p. 184), writing in 1889, says:—"Tous les observateurs qui se sont occupés de la conjugaison du *Paramæcium caudatum* ont signalé la petitesse de taille des individus accouplés." He goes on to say that he has never found them to exceed $225\ \mu$ in length, usually $180\ \mu$ to $210\ \mu$, while it is not rare to find non-conjugants attaining $300\ \mu$ or even $320\ \mu$; so that when Dr. Pearson states that "Dr. Pearl demonstrates for the first time that conjugant *Paramæcia* are differentiated from the non-conjugant population," he appears to be in error.

It may, however, be claimed that by the application of the biometrical method of dealing with the series of measurements he has given a more precise measure of their differentiation.

I would submit that Dr. Pearl's material and modes of procedure are singularly unfitted for yielding such a result.

In the first place, the specimens have been preserved and fixed, a process which every practical biologist knows to be attended with distortion.

They were prepared by different hands, partly by Dr. Pearl himself in Leipzig, partly by Prof. Worcester in America.

Dr. Pearl tells us (p. 377) that "in the measuring conjugant pairs were taken quite at random, and then in each case the two undistorted non-conjugant individuals which were lying nearest in the field of view of the microscope to the conjugant pair were measured."

Now let us consider what would happen with this mode of procedure. *Paramæcium*, as is well known, is not a symmetrical animal. It has been described as "slipper-shaped"—not a very good comparison, but it will serve to bring out the fact that the proportion of length and breadth presented to the observer will vary according to the aspect from which the individual is viewed. At whatever stage of the proceedings the *Paramæcia* took up the position on the slide in which they were measured, they must have sunk through a layer of fluid the depth of which was small, no doubt, but considerable in relation to their size. The conjugant pairs being attached mouth

to mouth would tend to settle on the broad base presented by the sides of the attached pair, so that one side of each rested on the slide while the other side would be directed to the observer. The non-conjugants might settle on any lateral aspect. Hence a larger proportion of conjugants would be measured in side view than of non-conjugants. This would be another source of error.

To illustrate the next point I shall refer to another ciliate infusorian, allied to *Paramæcium*, *Leucophrys patula*, to which I shall have to return later. It also was investigated by Maupas (*ibid.*, ser. 2, T. vi., p. 237, and T. vii., p. 250). The ordinary individuals of this species were found to vary in length from $80\ \mu$ to $150\ \mu$. They have a wide oesophageal recess bordered by vibratile lips (*cp.* ser. 2, T. vi., Plate xii., Figs. 1-8). The formation of the conjugants occurs by a series of divisions, with progressive reduction in size, of an ordinary individual and of the resulting fission products, giving rise to from eight to thirty-two little conjugants $50\ \mu$ to $60\ \mu$ in length, and so unlike the non-conjugant form that unless their mode of origin had been ascertained, Maupas says, they might be referred to a distinct genus. There are neither vibratile lips nor oesophageal recess, the mouth is closed, and their movements are much more active. Here, then, is a still more marked case of differentiation of gametes than that presented by *Paramæcium caudatum*.

Now the non-conjugant population of the latter species measured by Dr. Pearl to ascertain the range of their variability would include, not only ordinary individuals, but all stages of individuals in process of differentiation as gametes. The non-conjugants are a heterogeneous population; the conjugants are, on the other hand, approximately homogeneous. This appears to me another and grave source of error in his results on the degree of differentiation and variability of the conjugants.

Hence, though I am far from denying that it may be true, it appears to me that Dr. Pearl's conclusion is beset with several sources of error when he attempts to give a measure of the degree to which (p. 379) "conjugant individuals when compared with non-conjugants are found to be . . . less variable in both length and breadth."

I desire to do Dr. Pearl all the justice I can, and his case for homogamy in the conjugation of the gametes appears to me to rest on a sounder basis and to be of interest, though I am doubtful as to the validity of the explanation which he offers for this phenomenon; but that there is any analogy between it and assortative mating in man, as Dr. Pearl and Prof. Pearson conclude, seems to me problematical in the extreme. The phenomenon in man which is comparable with the conjugation of the differentiated gametes of *Paramæcium* is the union of the differentiated gametes of man, and I am not aware that it has been shown that there is any correlation between their external characters and the external characters of the human adult.

Similarly, the conclusion contained in Dr. Pearl's ninth and last heading appears to me altogether unsound. He says (p. 383), speaking of the differentiation of conjugants, "if the individual *Paramæcia* of a given race must conform to a definite and relatively fixed morphological type every time they conjugate, what they may acquire during fission generations is clearly of no particular account to the evolutionary history of the race in the long run." This is to ignore the conclusion to which Dr. Pearl's results point (though it had already been established by Maupas and others), that the conjugants are differentiated gametes. It is the nature of a gamete that it is able to transmit the characters of the organism from which it springs, although itself of a size and bodily shape wholly different from that organism. Are the gametes of *Leucophrys patula*, though unlike the ordinary individuals in size and other characters noted above, unable to give rise to like forms? As a matter of fact, if proof were needed, Maupas watched them in process of differentiation into ordinary individuals.

In my address at York I urged biometricians to make sure that the problems they seek to elucidate are sound from the biological point of view. When asked by Prof. Pearson for an instance of failure in this respect I gave

him, while away on my holiday, and in a private letter, Dr. Pearl's paper. He has now seen fit, although I twice asked him to wait for a full answer until my return to Cambridge, to challenge me to show in the pages of NATURE how my advice was applicable to that paper. I must leave your readers to judge how far I have succeeded in so doing.

The task has been far from an agreeable one. I should never have thought of singling Dr. Pearl's paper out for public criticism in this manner had I not been challenged to do so. I can only say that if he feels himself aggrieved at the result, he can be in no doubt whom he has to thank.

J. J. LISTER.

St. John's College, Cambridge, October 1.

Radium and Geology.

IN the Proceedings of the Royal Society for May and August there appeared important papers by the Hon. R. J. Strutt upon radium in the earth's crust and the earth's internal heat. Taking known values of the heat production of radium, per gram per second, assuming Lord Kelvin's estimate of the conductivity of rocks *in situ* and Prestwich's estimate of the temperature gradient at the surface, Mr. Strutt shows that, if the gradient expresses the outflow of heat due to radium in the earth, the radium must be confined to a comparatively thin crust, because his laboratory experiments prove that the smallest radium content existing in the rocks examined would give a much higher gradient than the one observed if the radium were distributed throughout the entire earth.

In the present connection the crust must be defined by the depth beyond which no heat is caused by radium. In these circumstances, if we adopt a certain temperature gradient at the surface, there is only one value of the radium content which will correspond to any assumed thickness of the crust, and there will also be one corresponding temperature at the bottom of the crust and throughout the interior. I have calculated these at intervals of five miles, both for Prestwich's estimate of the gradient, viz. 1° F. for 42.2 feet descent, and also for the more commonly accepted one of 1° F. for 60 feet.

Gradient 1° F. in 42.2 Feet.

Thickness of the crust in miles	Radium content per cubic centimetre	Temperature at bottom of crust, Cent.	Temperature at bottom of crust, Fah.
15	15.39×10^{-12}	519	966
20	11.55×10^{-12}	692	1277
25	9.13×10^{-12}	865	1589
30	7.70×10^{-12}	1038	1900
35	6.60×10^{-12}	1211	2211
40	5.77×10^{-12}	1384	2464
45	5.13×10^{-12}	1557	2834

Gradient 1° F. in 60 Feet.

Thickness of the crust in miles	Radium content per cubic centimetre	Temperature at bottom of crust, Cent.	Temperature at bottom of crust, Fah.
15	10.27×10^{-12}	363	676
20	8.08×10^{-12}	484	894
25	6.39×10^{-12}	606	1112
30	5.09×10^{-12}	727	1330
35	4.62×10^{-12}	848	1547
40	3.84×10^{-12}	969	1725
45	3.59×10^{-12}	1090	1984

From the above tables it appears that the radium contents corresponding to such values as are usually assigned to the thickness of the earth's crust by geologists and seismologists are well within the amounts contained in the

rocks examined by Mr. Strutt, and that consequently the surface gradient can be fairly accounted for by the theory. But we have also some indication of internal temperature from volcanic products. Prof. Bartoli found the temperature of lava issuing from Etna to be 1060° C. If this came up from beneath the crust it would correspond to a thickness of from thirty to forty miles, according to the rate of increase which we attribute to the gradient. So far all seems favourable to the theory.

Since any reasonable assumption for the mean radium content of the crust would supply sufficient heat to maintain the observed gradient, it follows that no heat can pass up from the interior, because, if it did, the gradient would be higher than it is. The conclusion would be that the earth is not a cooling body, and it is consequently reduced to a state of thermal stability.

Thus a fundamental belief of geologists is shattered at a blow. Sir A. Geikie writes in his chapter on dynamical geology that "it is useful to carry in mind the conception of a globe still intensely hot within, radiating heat into space, and consequently contracting in bulk." "Wide geographical areas are upraised or depressed." These changes of level are constantly going on, such as have been described by Prof. Hull and Dr. Spencer, and the recency of these movements shows that, if they are due to a cooling globe, that process is still in progress, and the primeval heat not yet exhausted. Although there may be differences of view as to the exact mode of its operation, yet it is not too much to assert that there is a consensus of opinion among geologists that the movements of the crust are chiefly attributable to the ultimate cause so concisely expressed by Sir A. Geikie.

It seems clear that one or other of these views concerning the internal heat of the earth must yield. They cannot both be correct; and if the radium theory is to hold the field, how are the movements of the earth's crust to be accounted for?

O. FISHER.

Graveley, Huntingdon, September 28.

If the internal heat of the earth is mainly due to the radium present therein, must we not assume that the same is the case with the moon? If such were the case, then the internal heat of the latter would be far greater than we have hitherto supposed, and it would be difficult to explain the lack of volcanic activity there.

The age of our satellite is not sufficient for us to assume that all the radium is dead or that none is being produced.

B. J. PALMER.

Technical Schools, Southend, October 4.

Vectors, &c., at the British Association.

IN the report (August 30) of the discussion on the use and notation of vector analysis at the British Association it is stated that I "deplored the substitution of vectors for quaternions." The statement is misleading, for was it not Hamilton more than any other single man who taught us how to use vectors in product and quotient combinations? What I did and do deplore is the substitution of non-quaternionic vector algebras in all their variety of notation for the Hamiltonian or quaternionic vector algebra—a very different thing.

I should like to add that (notation excepted) I was thoroughly in sympathy with all that Prof. Henrici said in opening the discussion. He showed admirably the consciousness of vector methods in attacking both geometrical and physical problems, and so far as he went in the limited time at his disposal there was absolutely nothing to choose between his mode of presentation and that which Hamilton himself might have adopted in the same situation. In his reply at the end of the discussion he pointed out that the quaternion, as a quantity, could be got quite easily from his system by taking the difference of his vector and scalar products. That, of course, is self-evident, but it does not seem to me to touch the real issue. It leaves his system still non-associative in vector products, and in higher applications, especially with the differential operator ∇ , this introduces difficulties which